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## RECENT STUDIES IN THE CORRELATION OF EYE MOVEMENT AND VISUAL PERCEPTION.[1]

BY RAYMOND DODGE,

*Wesleyan University.*

In two dissimilar lines of investigation the relation of the visual processes to eye movements has recently made notable advance. Both the Yale Psychological Studies[2] and the Psychology of Reading[3] by W. F. Dearborn publish a mass of unusually interesting experimental data bristling with unexpected problems. Each attempts to interpret the data of eye movement with reference to the complex facts of perception of a particular group of phenomena, while the Yale Studies seek a generalized statement of the relation of movement to perception as such. In both investigations the experimental methods are so abundantly justified by the results, that the technique itself must be regarded as one of their important contributions. Indeed, in view of the wealth of unsuspected variations in movement, which these methods have disclosed, it is doubtful if any merely introspective analysis of fixation, muscular balance, and eye movement deserves serious consideration.

The main advantages of the photographic registration of eye movements consist in its objectivity, its simplicity and accuracy, and the absence of discomfort either during the process or later. Both its objectivity and its accuracy contrast sharply with the older after-image methods. If, for example, the problem were to determine the position of the fixation points in reading, the after-image method would be limited by the span of memory, while any after-image sufficiently

[1] This number, dealing especially with experimental topics, has been prepared under the editorial care of Professor Raymond Dodge.

[2] Vol. VII., No. 1, Monograph Supplements of the PSYCHOLOGICAL REVIEW.

[3] Vol. XIII., No. 8, Columbia University Contributions to Philosophy and Psychology.

distinct to be serviceable would be a constant menace to clear vision, and its demands on the attention would more or less disturb the processes it was serving to investigate. This division of attention would be even more hazardous if the object fixated were a geometrical illusion. Similarly the invaluable method of direct observation is limited by the memory span of the observer, and is irregularly interrupted by the pursuit movements of the observing eye. Doubtless both the after-image method and the method of direct observation will continue to be useful, but they will serve us chiefly either in preliminary tests for the direction of more exact experiment, or in the specific application of established generalizations. Reasonable demands for safety and convenience preclude mechanical registration even if the effect of the abnormal muscular load could be disregarded. For the present at least some form of photographic registration must be relied on for knowledge of the facts of eye movement, and the present papers amply justify its use.

Each of the two publications describes its own variety of photographic registering apparatus, developed into a practical method. Since neither system is perfect in some more or less important details, and since the general procedure is suffering modification and refinement, it may not be out of place to indicate some of the main sources of error in the two types, and at the same time to point out the peculiar advantages of each.

Dearborn used the earlier and simpler method. He photographed the apparent movement of the reflection of a bright light from the eccentric surface of the cornea. As the image of this reflection fell on a moving plate he obtained directly a continuous curve of eye movement. But since one of the coördinates of the curve represents elapsed time, the other can adequately represent eye movement in only a single plane. In the case of reading movements that plane is naturally horizontal and the chief limitation of the method is comparatively insignificant. Nevertheless, even in the investigation of reading, it would be valuable to know the character and extent of the vertical excursions. Furthermore, one can never be sure that a comparatively small irregularity of the curve does not represent a considerable excursion in a plane removed from the horizontal. Dearborn is thoroughly alive to this limitation, but fortunately the main problems in the physiology of reading may abstract from the vertical movements altogether. On the other hand, as Judd rightly points out, a study of the complex eye movements involved in the perception of a geometrical illusion may not ignore movements of the eyes in any plane. Judd overcame the

limitation of the older method by taking a rapid succession of discrete photographs with a kinetoscope camera. The position of the eye in each photograph may be determined by reference to points on a base line carried on a spectacle frame. It should be pointed out in passing that whenever worth while, Dearborn's apparatus could be adapted to measure the movements of the eyes equally in all planes if a base line were employed with intermittent illumination. Doubtless this modification of the apparatus will be adopted in future. Holt's use of the alternating current arc light suggests the simplest possible source of intermittent illumination. By this method each discrete record could be referred to its base line just as in the kinetoscope records, while the falling plate apparatus has advantages of its own in matters of expense, simplicity of operation, rigidity, and in the perspicuity of its results.

The apparent movement of the corneal reflection was used by Dearborn as a measure of eye movement. The device has at least one obvious advantage: the subject is not emotionally disturbed by the introduction of foreign matter into the eye, and the method may be employed without special practice with children as well as with adults. Moreover, the photographic images of the corneal reflection are sharper and brighter than the images of any pigment thus far used. This means that for any required density of negative the illumination may be relatively less than is possible with pigments. The legitimacy of the method depends on certain precautions. The source of illumination and the camera should be placed symmetrically on either side of the object of regard, and the excursions of the eye should never be so great as to bring the corneal reflection off the relatively true optical surface found in normal eyes to cover more than half of the whole area of the cornea, approximately concentric with the visual axis. Anything more than this must be subjected to empirical calibration. These precautions Dearborn faithfully observes. Judd points out that every change in the axis of rotation of the eye must produce a corresponding change in the ratio between the apparent movement of the corneal reflection and the movement of a point across the retina. This is obviously true, but it is equally true of every photographic or other method of direct registration. The use of a pigment spot certainly does not obviate it. Probably the only absolutely safe method in this respect is the after-image method, but that is open to much graver errors. After all is said these possible sources of error are intrinsically small, and relative to the errors of measurement and the present profitable use of the data they are insignificant.

In several respects, however, I regard Judd's brilliant device for attaching an innocuous pigmented point to the cornea as a matter of the utmost importance to some forms of investigation. It permits the accurate measurement of longer eye movements than the corneal reflection permits, while for the shortest movements it gives photographic records approximately twice the amplitude of those from the corneal reflection. Finally the important, but thus far unfortunately ignored, element of torsion can be measured objectively, as far as I am aware, only by the pigment method.

The chief difficulty common to all objective methods is the interplay between head and eye movements. No practicable means has yet been discovered of holding the head absolutely quiet. Even the most elaborate combination of clamps and mouth plates holds the head only approximately rigid. Still more disquieting, since there is no obvious means of measuring them, are the effects of coördinate compensatory impulses. It has been demonstrated that accurately adjusted movements of the eyes accompany some specific movements of the head and trunk without measurable reaction time. Such eye movements are not dependent on actual head movements, but on the motor conditions which would lead to head movement. Consequently, even if the actual head movement is quite inhibited by mechanical means, while the eyes are still free to move, the involuntary and normally unconscious compensatory eye movements, coördinate with the mechanically inhibited head movement, may well appear as a real disturbance of fixation. There is some indirect evidence that they do.

The extent of actual head movement may be measured accurately by reference to a fixed base line independent of the head, and this precaution deserves place in all future records. The control used in the Yale Studies does not seem adequate to me on mathematical grounds. They relied on noting changes in the mutual relation of points on the base line attached to and moving with the head. But since these will change only as the sines of the angle of head movement, their increment may be considerably within the limits of measurement for angles of head movement which occasion entirely measurable compensatory eye movements. Suppose, for example, a total movement of the base line attached to the head of .5 mm. If the head be regarded as moving on an axis 150 mm. from the spectacle frame, the apparent relative change in the distance between two points, 25 mm. apart on the base line, will be roughly 150 (sine 10° — (sine 10° 12′ — sine 0° 12′)) = .0081 mm. This is far below the limit of measurement in the Yale Studies, while the consequent total apparent displacement of

the eye is more than ten times the unit of reliable measurement. I think that the general importance of working with a control base line independent of the head is incontestable. Dearborn frankly leaves the entire responsibility for eliminating disturbing head movements to the rigidity of his head rest. While this probably leads to no large error in his conclusions, it has obviously handicapped him in the exploitation of his splendid collection of data.

A general résumé of the contents of the Yale Studies, published elsewhere in this number of the BULLETIN, permits me to restrict my discussion to a few special features.

McAllister's analysis of the process of fixation rests on data of unusual theoretical interest. Our traditional conception of the fixation of a point adjusts itself with difficulty to an involuntary wandering of the fixation point over an area whose diameter may be two degrees. Under such circumstances fixation is a misnomer. The functional center of the retina must be regarded as a variable whose mode of variation becomes a serious problem of physiological optics. Evidence of gross errors of coördination of the two eyes complicates the problem, but proves at the same time that the records cannot be referred to head movements, either actual or inhibited. Variations in the distribution of the errors occasioned by experimentally modifying the environment of the fixation point show a general correspondence to the character of the surrounding lines, but they throw comparatively little light on the character of the eye movements in viewing complex figures. McAllister's diagrams are cruel.

The records of eye movements across figures producing geometrical illusions present a mass of data complex, paradoxical, and altogether difficult of interpretation. The two eyes of the same observer never move in exactly the same way at the same time; while no two successive eye movements across a figure have more than a remote similarity. The records unequivocally justify Judd's negative conclusion, that the character of the illusion is not determined by sensations of eye movement either immediate or revived. Indeed, in view of the relative consistency of the illusion and of the extreme variation in the recorded eye movements, any attempt to correlate sensation and movement seems little short of hopeless. Judd's discussion of this general problem, under the title 'Movement and Consciousness,' is the soul of the series. The movement sensation hypothesis he definitely abandons, at least with respect to vision. On the other hand, the general relation between movement and attention, as well as certain specific typical differences in eye movements when

the illusion is marked and when it has disappeared, make it obvious that there is some sort of relationship between them. On general theoretical, as well as on experimental grounds, Judd finds a satisfactory explanation of their correlation by regarding movement as an expression of the interrelation between the various sensory factors. The central motor organization is not to be regarded as a factor in the sensory process but a bond of unity — a central mode of organization. Whatever sensory impulses can be brought into coördination and equilibrium by a simple act will be grouped together in space and time. An illusion is not due then to any sensory factors but to an illusory relation, and that relation is a central motor process.

Coördination of some kind between sensation and movement there surely is, but to make movement the sole coördinating fact of consciousness seems to the reviewer to go too far, unless, indeed, one goes still farther and regards all sensation as a form of movement, when the peculiar significance of the distinction would break down. There is indeed good evidence that a motor process does under some circumstances unite sensory factors into a whole. On the other hand it seems equally true that a presentational fact may be the bond of unity, coördinating numerous motor responses into a single act. Thus whether a broken sweep of the eye, with its half dozen constituent sweeps and corrective movements, is a single act or a number of acts, depends on some fact of organization outside the mere motor phenomena as such.

Dearborn's problems in the correlation of eye movements and perception are qualitative rather than quantitative and specific rather than general. While corroborating the general facts of the physiology of reading discovered by Prof. Erdmann and his collaborators, and independently by Huey at Clark, Dearborn found that the number of fixations per line of print varies with the ocular motor habits of the reader, but that in general short lines required relatively more fixations than long ones. Familiarity with the text tended not only to reduce the number of fixations but also to eliminate their irregularities. There was also a marked tendency in all reading which presented no particular difficulties to establish 'short-lived motor habits' with regard both to the number of fixations and to the inter-fixation movements. The influence of these habits was particularly apparent in the false movements, frequently found where the lines were of unequal length or were indented unequally. Moreover, the frequent refixations which occur during the reading of the first few lines of a passage seem to indicate a tardiness in the formation of such habits. In view of these

facts Dearborn points out with right that most primers and illustrated readers make early instruction in reading unnecessarily difficult by long and irregular lines.

More or less frequently interspersed in the regular alternation of fixation pauses and rapid eye movements, Dearborn found a slow drifting eye movement whose amplitude often exceeded two rapid movements and whose duration was from ten to twenty times that of rapid eye movements of similar amplitude. A slow movement of this kind uniformly took the place of a fixation pause between two rapid movements and apparently usurped its function as a moment of clear vision. Dearborn interprets these slow movements as an involuntary eye drift due to peripherally directed attention. They revived for him the discussion of the successive apprehension of printed words. Surely if they were unequivocal they would constitute the best experimental evidence at hand for the theory of Wundt and Zeitler. But, as Dearborn pointed out, they are far from being unequivocal. They occur from right to left as well as from left to right. Moreover, they are only occasional with any subject and they are rare with some. These facts suggest to the reviewer the possibility of a purely mechanical explanation. If they were to be regarded as evidence of the successive apprehension of a word, by the same token the more numerous real fixation pauses must indicate a totally different sort of process, while the retrograde movements would indicate the successive apprehension of a word backwards, a rather difficult process. Finally, tachistoscopic reading showed that whenever the slow movements did occur they were quite independent in time of the exposure of the word. The reviewer has described two types of eye movements which correspond closely with these slow movements both in velocity and in function. On the one hand they resemble compensatory eye movements and might arise as the accompaniment of incipient head movements which are mechanically restrained by the heavy head-rest. They also resemble the simple corrections of binocular incoördination. The only test for the latter would be binocular registration. A fair test for the former would be a few photographs without rigid head-rest, showing free head movements as well as the eye movements.

The duration of the fixation pauses varied widely with different individuals and with the same individual under different circumstances. While in general they were well above the recorded reaction time of the eye, some of them were materially shorter. The first fixations in each line averaged longer than any of the others for all subjects. Dearborn's hypothesis to account for this interesting fact is one of the

most important of recent contributions to the psychology of reading. He holds that the long initial fixation involves a general survey of the whole line and especially of that part of the line covered by the next two or three fixations which are regularly very short. This explains those fixations which are shorter than a normal reaction interval, as representing only a part of the true reaction interval. The question how much of the process of apprehension is subserved by this pre-fixational perception is theoretically and practically of the greatest importance. The large number of fixations necessary in reading a group of numerals contrasted significantly with the number of fixations in reading a line of words.

An analysis of the factors which determine the position of the point of regard at each fixation is the most difficult task of the monograph, and perhaps also the most important. Short connective and non-substantive words, prepositional phrases, and relative clauses are most consistently exacting. Familiar phrases and familiar words, however long, may be read from a single fixation. General training in reading and the arrangement of the matter read, as for example, the length of the line, the position of phrases within the line, the development of motor habits, and the attention of the reader to his work, all appeared to influence the points of fixation. Dearborn's practical and pedagogical suggestions are well within the limits of legitimate inference from his data.

# PSYCHOLOGICAL LITERATURE.

## EXPERIMENTAL PSYCHOLOGY.

*Experimental Psychology.* EDWARD BRADFORD TITCHENER. Vol. II., Quantitative Experiments. The Macmillan Company, New York, 1905. Part I., Student's Manual ($1.50); Part II., Instructor's Manual ($2.50).

These two Parts (separately bound) of Volume II. are the promised conclusion of Professor Titchener's manual of experimental psychology. Their appearance at length, even more than that of the foregoing two manuals of Qualitative Experiments, must highly gratify and encourage all English-speaking psychologists who look for an end to the present era of ill-considered and go-as-you-please experimentalizing. For it is to be admitted that psychology as a whole is looked at askance, and not without reason, by chemists and physicists; and that American, and such as there is of English psychology is too often merely an object of ridicule for exact scientists everywhere. It is not a pleasant thought, yet it is true that our psychological publications evince a general dissoluteness of thought, a lack of discipline, a disunitedness of purpose. Far too often we know neither why nor whither we are working. And while on the one hand we abound in experimental essays that are naïve and casual to the point of inanity, we balance this off, on the other hand, with a pretentious and futile pedantry; aimless investigations tricked out with an ignorant flourish of mathematics. Unpretentiousness, discipline, erudition, breadth, and logical pregnancy we sadly lack. Psychology at large seems scarcely to know its goal, but we in America have hardly any sound tradition or concertedness of purpose. We speak a language of which we have not yet mastered the grammar. Against all this a better counter-sway could scarcely be devised than Professor Titchener's present two volumes. They constitute the admirable and indispensable grammar.

If this work (the two Parts of the second volume) is a grammar, it follows that the student who works through this Manual is to gain merely discipline. Dry it will be pronounced by the student who wishes to pluck the fruit without having sown the seed. And it is to be admitted that the Manual does not disclose at every turn enticing vistas of research: the student will not be intoxicated with a desire to

pry further into the secret mechanism of mind and brain. For in this respect there is a subtle distinction between books — well illustrated, for example, by Nagel's Handbuch (volume three) where every page suggests some serious problem yet to be resolved, as compared with Schäfer's Text-Book (volume two) in which, excellent book though it is, the reader is less reminded of what remains to be discovered. Grammar, however, is confessedly a dry topic, albeit wholesome and necessary. And just as the text-book of grammar does properly not inspire the student to flights of eloquence, so, it seems to the reviewer, a drill-manual in psychology does well to make no concessions to inattention or listlessness on the part of the student. The phenomena themselves should inspire the enthusiasm for research and explanation. And the teacher who himself finds the Manual uninteresting, is exposed to a number of unfavorable comments: for the instructor's volume (Part II.) is a compact store of painstaking and minute historical exposition, and subtle and well-weighed criticism, of the methods of psychology.

A manual of 'quantitative' psychology must stand or fall on one issue — its answer to the question as to what the 'quantities' to be studied are. Are they psychical, or are they physical, or are they both? And if psychical, how are they to be defined? Here Professor Titchener has unquestionably, in the reviewer's opinion, taken the right point of view. Physical quantities are to be correlated with psychical quantities, and these latter are not sensations, but sensation-*differences* — the intervals or distances between sensations that form a linear series. The just-perceptible, or over-perceptible differences are the psychic quantities. The history and present state of this fundamental and much-disputed postulate are set forth in section 6 of the Instructor's Manual. The 'view of mental measurement as distance measurement,' adopted by Professor Titchener, is ascribed to J. R. L. Delbœuf; but it has also found advocates 'in one form or another, in whole or in part,' in 'Wundt, Boas, Stumpf, Ebbinghaus, James, Meinong, Höfler, Stout, and G. E. Müller.' Whether the word 'quantity' or 'magnitude' is allowed in psychology, or not, is of course a minor matter, and the history sufficiently shows that the disputes have chiefly centered around a word of which neither side had in mind an exact definition; 'commutative' and 'associative' laws are seemingly unfamiliar to psychologists. The vital point is that sensations form ordered series (pitches, color-shadings, et cet.), and that of any one element of such series consciousness can predicate 'Present' or 'Absent'; that of any two elements present, consciousness can predicate

'Same' (*i. e.*, =) or 'Different'; and that if any three (different) elements are present, consciousness can predicate of the two *differences* between them, 'Greater,' 'Same' (=), or 'Less.' It might be hoped that few psychologists would dispute this. Whether, now, such series of elements involve 'magnitude' is a question for mathematics: and it just happens that in mathematics they do involve magnitude. Oddly enough, Professor Titchener denies the final importance of the predicates 'greater' and 'less.' "The questions asked of consciousness are, in the last analysis, two only: 'Present or absent?' and 'Same or different?' For instance: Do you still hear a tone? or: Is this weight heavier [!] than this other, or lighter than it, or just as heavy?" Surely the answer 'heavi*er*' or 'light*er*' is an answer to neither of the questions 'Present or absent?' 'Same or different?' And in the reviewer's opinion the question 'Greater (*i. e.*, more) or less?' is not resolvable into the two others. Were the judgments 'More,' 'Equal,' and 'Less' impossible, either of the sensations themselves, or else of the intervals between them, the sensations would not constitute ordered series and would not involve magnitude.

The main body of the Manual consists of twenty-seven experiments distributed between sight, hearing, pressure sense, active muscle sense, simple and compound reactions, and time sense; and variously illustrating Weber's Law and all of the psycho-physic methods. It was necessary to make extensive use of mathematics, and in view of the various degrees of preparation met with in students of psychology this has been a delicate task: for "while the professor himself knows the value of the mathematical attitude for investigation, * * * he cannot say that the results of the application of mathematics to psychology are, at the present day, such as to warrant the exclusion of the non-mathematical student from his laboratory. It is, indeed, doubtful whether such exclusion will ever be warranted." The task has been admirably accomplished, and for the most part the mathematical procedures and formulæ involved are so clearly explained that any student of ordinary intelligence can easily understand them, even if he has studied no more than arithmetic and algebra. He is, moreover, led up gradually from the simple to the complex applications. In naming the psycho-physic methods Professor Titchener has departed from the traditional, mainly Wundtian, designations. The Method of Minimal Changes has become the Method of Limits ('Grenzmethode' of Kraepelin and Müller) because "it is better than the name 'minimal changes,' since it brings out the fact that the series stops short at a boundary, at the turning point of judgment." It may

be doubted whether the student will find this name as natural and significant as the old. Far better is the change from Method of Right and Wrong Cases to Method of Constant R. (Reiz). The true difficulty lies in the classification of the methods, and until that is adjusted no change of nomenclature will much avail. Thus the *Method* of Equal Sense Distances (*i. e.*, Method of Mean Gradations) turns out to be a *problem*, and the student is told (Part I., p. 82), "The most natural way, perhaps, is to apply to our present problem the principle of the method of limits." Equal sense distances are in fact a problem, and it is preserving an old equivocation to call that problem a method. Such equivocations abound in Wundt.

Some minor details call for comment. In the method of limits, the experimenter is told to stop at 'the turning point of judgment.' Thus in finding a difference-limen, when, say, the variable stimuli in ascending series are compared with a standard, the judgments on the former are 'less,' 'less,' et cet., until finally the judgment is given 'equal' or 'doubtful.' Here the comparisons stop; and a new series is commenced with the variable equal to the standard, and descending. Now it may be questioned whether the comparisons should stop until the variable is objectively equal to or even greater than, the standard; although then, certainly, the results are more complicated to evaluate. To be sure, Professor Titchener justifies his procedure very subtly (Part II., p. 109), and Wundt's precaution to increase the variable 'by a few more steps' beyond the turning-point of judgment, in order 'to be on the safe side,' simply makes the whole procedure inconsistent. Nevertheless the reviewer believes that there is empirical evidence to show that by the method here prescribed the difference-limen will vary appreciably (and directly) with the *size* of the steps arbitrarily chosen between the variable stimuli. The author's injunction that the steps must always be 'very small' will only partly reduce the error.

Under the Method of Constant Stimuli, the distribution of results is meant to be illustrated by a curve (Part I., p. 94) the relation of which to the results themselves, given on the previous page, can by no means be clear to the student; at least it is not clear to the reviewer. On page 107 similar data are illustrated by a similar curve. As a preface to the reaction experiments, there is a little treatise on the electric current with special reference to its use in the laboratory. This is for the most part clear and excellent, but it could be improved in places. Thus on page 129 (Part I.) the description of a shunt, although accurate, will certainly puzzle students: and from page 131 they will be likely to conclude that large incandescent lamps have

generally more resistance than small ones; on the following page it is stated of apparatus on a shunt that, "The closure of an apparatus circuit must always lessen the resistance of the lamp or strip with which it is in parallel, * * *" The author is generous and nice to a fault in giving credit for the lamp battery to A. Wright and E. W. Scripture: it is to be hoped that the patent rights on the introduction in psychology of simple devices long familiar in physics, will presently expire. Lastly, there are the inevitable typographical errors, very few in number, however, except in the German quotations; and nowhere important, unless it be in the substitution of 'rapidity' for 'slowness' (Part. I., p. 43), and the omission of an '$x$' in the fifth equation on page 50.

The student who shall have worked through with care the experiments of this Manual will be indeed prepared to proceed to an original research in psychology. The work amply deserves to be adopted, for firstly, it is specifically planned to afford just that discipline that American psychology to-day lacks, and secondly, this plan is worked out to the last practical detail with remarkable skill and a prodigious amount of care. In conclusion it should be said that the Instructor's Manual will itself be invaluable to the psychologist, whether a teacher or not, by reason of some hundred compact and scholarly bibliographies, and some forty or fifty minute discussions of important psychological topics, which for their sheer erudition and general level-headedness and breadth of grasp can scarcely be surpassed.

EDWIN B. HOLT.

HARVARD UNIVERSITY.

## MONOGRAPHS.

*Psychologische Studien.* THEODOR LIPPS. (2te umgearb. u. erweit. Aufl.) Leipzig, 1905. Pp. 287.

The kind of psychology which Lipps always produces in his writings is one which is little cultivated in America. It is descriptive and systematic psychology of an extreme type. One cannot wholly refuse to overlook its references to empirical facts, for in all his writings Lipps has exhibited a wide acquaintance with the facts germane to his discussion. But the empirical facts are always marshaled around some central definition or theory. The theory or postulate is elevated to a position of such importance and its support seems to be a matter of such cardinal moment, that a kind of psychological dialectic results, which leaves on the reader the impression that the whole treatment is forced and formal. There is a certain strict logical co-

herency in such writing, but the price paid for this coherency is often high and the details of the discussion are devoted to defense of system rather than to statements of new and suggestive points of view. Take, for example, the second paper of the present series. The question is, how can musical harmony and dissonance be explained? The answer is that every musical tone has a characteristic which may be called 'tone rhythm.' This tone rhythm is a psychical quality dependent on the number of vibrations in the physical stimulus and the consequent capacity of the mental experience which is conditioned by this stimulus to form combinations with other tones. The evidence that every tone has tone rhythm is to be found in the fact that tones show the capacity for combination which is referred to in the definition, and consequently the capacity must exist unconsciously if not consciously and must be made the basis of any explanation of harmony. After stating this principle or definition, Lipps proceeds to the discussion of the theories of Helmholtz, Krueger, Stumpf and Wundt; and later, in applying the theory to melody, takes up the theory of Max Meyer. In his criticism of these other writers he has a distinct dialectic advantage. His tone rhythm is a flexible concept capable of including all that he discovers in combinations. Whenever he finds the particular writer whom he is criticising in any difficulty he offers his universal solvent, which is the more useful because it is a theoretical characteristic made to systematize the facts in hand.

Again, take the first paper of the present series. The first question here is how are the points in the field of vision arranged in a spacial order. The answer is that because the world is regular and orderly in its presentation we each develop an orderly form for the interpretation of experience. Eye movements do not explain this regularity, says Lipps, for they do not correspond in their characteristics to the regularity of the field of vision. The nativistic theory is inadequate because it leaves no room for the progressive development of experience in its adaptation to the regularity of presented experience. Disposing thus of theories which attempt to explain the facts, Lipps falls back on a theoretical relation which is less objectionable just because it does nothing but describe in a vague general way what seems to be clearly observed.

Another illustration of this type of psychology is found in the third discussion in the first paper. Visual depth is explained as the result of a recognition in experience of the fact that certain phases of experience do not belong in the flat extension of plane space. We get at times experience from the side of a house and again later we see

the front without being able to see the side. The side is, therefore, recognized as an experience differing in kind from that which we get from the flat front. This is sufficient cause for the rise in visual perception of the third dimension. Here again one is disposed to offer the comment that while the statement is undoubtedly true, it does not seem to advance the discussion.

The three studies which constitute the present volume are on the following subjects: Visual space, the nature of musical consonance and dissonance, the psychical law of relativity and Weber's Law. All are elaborations of principles familiar to readers of Lipps' writings and are for the most part polemical and discursive, as above described.

CHARLES H. JUDD.

YALE UNIVERSITY.

*Yale Psychological Studies, New Series, Vol. I., No. 1.* Monograph Supplement, PSYCHOLOGICAL REVIEW, Vol. VII., No. 1, March, 1905. New York, Macmillan Co. Pp. 226.

'General Introduction to a Series of Studies of Eye Movements by Means of Kinetoscope Photographs,' Chas. H. Judd, Cloyd N. McAllister and W. M. Steele.

This paper describes a method used in the Yale Laboratory for recording eye movements. The difficulty which former investigators have had in getting a fixed spot on the eye, which will show in a photograph, has been overcome by the use of small pieces of Chinese white coated in paraffine. These are placed upon the cornea of the eye. Fixed points of reference are obtained by the use of wire spectacles supplied with steel beads upon the rims and worn by the subject to be photographed. Devices for keeping the head of the subject always in the same position and for controlling the light are also described in the paper. The camera used was the Edison kinetoscope camera, which is described. The reading of the records was effected by projecting the photographs by means of an ordinary electric lantern upon a sliding board covered with paper.

'The Fixation of Points in the Visual Field,' Cloyd N. McAllister.

The method above described was used to determine (1) how the eye behaves when an observer is consciously fixating a point; (2) how the eye moves from one point of fixation to another. In fixating a point it was found that the eye does not remain absolutely still but wanders around a small area in the region of the point. Lines in the neighborhood of the point of fixation modify in a definite and positive manner the character and size of the fixation area. Whether during

periods of fixation or movement along a line, the two eyes are not exactly coördinated in their movements. The eye usually moves along a straight line accurately the first time the movement is made and in any case does so after a few trials.

'The Müller-Lyer Illusion,' Chas. H. Judd.

'The Poggendorff Illusion,' E. H. Cameron and W. M. Steele.

'The Zöllner Illusion,' Chas. H. Judd and Henry C. Courten.

All three of the above papers describe methods for the quantitative determination of the illusions named in the titles. A continued series of determinations was made with each of these illusions and in each case the illusion tended to disappear with practice. Photographic records of eye movements made in looking at these illusions showed a significant relation in every case between the movement of the eyes and the nature of the illusion. There is a change in the character of the movement of the eyes, corresponding to the change in perception effected by practice.

'Analysis of Reaction Movements,' Chas. H. Judd, Cloyd N. McAllister and W. M. Steele.

A method is here described for the analysis of reaction movements. By means of a system of spring and lever attachments to the reaction key, which allowed a record of the movements of the hand of the reactor before the reaction was actually made, various types of reaction in different individuals and in the same individuals were discovered. It was shown that in no case is a reaction a simple process, but that it always consists of a complex movement bearing a determinate relation to the state of preparation in view of the warning signal.

'Practice Without Knowledge of Results,' Chas. H. Judd.

The experiments here reported were made for the purpose of showing the effect of acquiring new habits when the subject is ignorant of his habit and its effects. The subject was seated at a table and lines running at various angles, upward and downward, were placed at his left, so that he could see them. The part of the table at his right was protected from his gaze by means of a screen, and he was asked to make a dot on this side of the table corresponding to the projection of the line. In the first series of experiments this was done for each of the lines — nine times each day for ten days. The practice brought about little if any change in the amount of error in each case. Two lines only were then chosen with which to work — the one running upward at an angle of 60° and the other downward at an angle of 45°. In the case of the former, the subject was now allowed to pull aside the screen and see the paper on which the dot was to be located. The

screen was then replaced and the subject made five successive markings. He was then again allowed to remove the screen. Proceeding in this way a noticeable improvement was made, the error tending to disappear entirely. The error in the case of the second line, however, on which practice was carried on at the same time but without removal of the screen, tended to increase rather than decrease. The new habit tended also to increase the error in connection with the others of the original lines. Other variations were introduced which showed conclusively that an acquired habit is often misapplied in connection with related cases and is also often very difficult to break down.

'Movement and Consciousness,' Chas. H. Judd.

In the last paper of the series, which is theoretical, Professor Judd shows that all the other papers deal, in one way or another, with the same problem, viz., the relation between bodily movements and consciousness. Following the lines laid down by Münsterberg, and especially McDougall and Dewey, he seeks to apply the coördination theory to the results of the experiments recorded in the earlier papers. According to this theory, 'the quale of any experience is equally determined by both the sensory and the response phases of a given coördination.' In the reaction experiments, instead of speaking of a stimulus to react entering a subject's consciousness and causing movement of a certain kind, we should speak of a certain state of preparation which is at once a state of sensory content and motor strain, which is changed into a new experience where both these factors are present. So, too, in the cases of the illusions — the false perception is not adequately described as dependent upon certain movement sensations, but it is a moving equilibrium, a disturbance of balance, in a process including both sensory and motor content.

E. H. Cameron.

Yale University.

## ILLUSIONS.

*Zur Verständigung über die geometrisch-optischen Täuschungen.* Theodor Lipps. Zeitsch. f. Psychol. u. Physiol. der Sinn., 1905, XXXVIII, 241–258.

The author discusses the 'consciousness of the size' of an object when the latter is apprehended as a whole and as a unitary linear system; the nature of the apperceptive activity connected with its apprehension, both in respect to its character as 'Einfühlung' and as a simple expanding and contracting activity; the ways in which this contraction and expansion cause illusions, with particular application to the Müller-Lyer and the 'vertical' (again introducing the concept

of 'gravity' or 'weight') illusions. He concedes the justice of the criticisms urged against his resort to the 'unconscious,' criticises certain interpretations of his theory of *Einfühlung* (a word used in the sense of the Vischers), criticises the theory of motor impulses or eye movements, applies his theory to explain certain empirical results of Vittori, and the analogous tactual illusions of Pearce, and, finally, expresses the conviction that, while the theory may be lacking in detailed application, it will be found equal to all emergencies.

We may substitute the concept of quantity for that of size, since any perception of magnitude gives my activity of apprehension a quantitative determination — a definite 'quantity experience.' The quantity (loudness) of a tone resolves itself into its 'impression ability'; the quantity of an extent consists in that it has the ability to determine a definite *Weite* of my apprehension activity. The immediate impression of the magnitude of an object when considered as a *whole*, then, depends upon the intensity and range which it demands of my activity of apprehension. When this is demanded in too great a measure the object is too great for the immediate impression.

Now, there is no whole of extension not made such by my synthesizing activity, hence my apprehension activity is really an *apperceptive* activity; a certain magnitude of the latter enters as an integrating element and the extent of this is determined by the 'Blick des geistigen Auges.' The expansion and contraction of the activity of the latter are prerequisites to the perception of an extended whole.

This apperceptive activity, which is experienced simply as a spreading out and narrowing, is, however, executed upon the suggestion of the lines; it is a two-fold property of the lines and belongs to their 'Eigenart.' Contrariwise, these two activities — expansion and contraction — are a matter of apperceptive 'feeling-in'; they are 'felt-into' the lines so intimately that they are not immediately experienced as apperceptive activities but as activities of the objects themselves — whence we may not refer them to 'associations,' least of all to associations between spatial forms and representations of powers or activities.

How, then, does the illusion arise in the Müller-Lyer figure? The perception of the figure requires a thoroughgoing apperceptive activity, in order that the main line and the obliques shall be apprehended as a unitary linear system. The expansion activity of the main line, which stops at its end points, is expanded still farther when the obliques point outward, whence an overestimation. But when the obliques point inward the expanding activity is turned back upon

itself, in order that the obliques may be embraced in the unity of the figure, whence an underestimation. It is the apperceptive activity which goes outside the main line in both cases and unifies the figure. In the case of the vertical, which is complicated with a 'Natureinfühlung,' there is a tendency toward the diminution of the range of the 'inner regard.' But I must give my apperceptive activity a reach corresponding to the actual length of the line, in opposition to this tendency of contraction (gravity, the tendency *felt-into* an object, to diminish the distance between it and that which is under it), and to do this requires a more intense activity (than in the case of horizontals); whence the overestimation of the vertical. The activity is *felt-into* the line, and is therefore thought to exist in the line itself.

It is doubtful whether many investigators of illusions will be content with having the factor of eye movements or movement tendencies resolved into 'apperceptive' or 'apprehension' activities with which, by definition, they are connected. Experiments seem to show that the movements actually occur in many cases; even if they only rise to the order of reflexes they may contribute essential sensation data. The movements of the eyeball can scarcely be resolved into 'movements of the psychical eye' (whatever that may mean). And, while there may be doubts as to the function of eye movements, Lipps' theory, if valid, must account for a considerable array of empirical facts which seem to be better accounted for by appeal to various sensation factors; *e. g.*, differences between the retinal meridians, between the two eyes, between different positions, distances, fixations, obliquities of lines, illuminations, motions, physical contrivances used, regularity in the presentations, etc. The consideration of these and other equally important factors and details in itself throws grave doubt upon the value of the theory. This aside, there are illusions (*e. g.*, the reversible) to which the application of the basal tendencies involved could only be metaphorical.

In the opinion of the reviewer, therefore, while Lipps has done much in this article to remove some of the obscurities which surrounded his earlier formulation, the value of the theory of Einfühlung and apperceptive activities is æsthetic; it does indeed enunciate a truth of general psychological validity, but cannot give a specific and complete account of an illusory perception: perhaps not of a single case. At the same time it is judicious, in the face of recent empirical results, not to be overhasty in pronouncing the theory altogether sterile; the battle now seems to be drawn, not so much as between the recog-

nition of peripheral and central factors, but as between the primary and secondary rôle of each. The occasional emphasis which the author lays upon the need of regarding the objective figure itself and its details, and his final emphasis upon the two-fold nature of the factors involved in the perception, are features which especially commend themselves to the reviewer.

J. E. WALLACE WALLIN.

PRINCETON UNIVERSITY.

*Optical Illusions of Reversible Perspective: a Volume of Historical and Experimental Researches.* J. E. WALLACE WALLIN. Princeton, N. J., published by the author, 1905. Pp. vi + 330.

The guiding principles for the historical portion of Dr. Wallin's book are set forth in words that may be quoted in illustration of the spirit, as well as of the literary style, of the volume. Usually, says our author, 'the rule is to select such material as offers palpable contrasts, as is deemed germane to a certain standpoint, or of far-reaching or essential importance.' Such a rule leads to 'all the imperfections attaching to the rehash: hyper-condensation of thought and exclusion of detail (albeit the statements are verbose), historical infidelity, non-genetic treatment.' Far from following such vicious ways as these, our author aims to give 'an exhaustive digest of all that has been said and done along the subjects treated, so far as the material has come to the attention of, or has been accessible to the writer in a somewhat extensive search in several libraries.' He "follows no abstract ideal of an 'ought' or 'should be' of selection, but attempts a faithful review of the 'has beens.'" 'Developmental stages in science'—and his book seems to have imitated their ways—'proceed in a blended zigzag, tortuous, helical, dragnet fashion.'

His attempt at "a faithful review of the 'has beens'" hardly brings results that will commend his method to many; the reader often tries in vain to get a clear idea of the contribution of some particular person, or of the connection of his work with that of others. The bibliographical references, however, will be helpful to one who wishes to review the field.

The experimental portion of the book is better, and in a number of particulars we have additions to our knowledge of perspective reversion. The more important points which the experiments bring out seem to be: that for very brief exposures there is generally a particular direction of perspective that is of more frequent occurrence; that the preliminary intention to see the one or the other relief does influence the direction of the perspective, without fully controlling it, and that

continued practice in attempts to influence the direction of the illusion by means of the will or the imagination increases the success of these efforts; that suggesting the one or the other form of perspective is of influence; that ocular fixation is important in determining the relief, although of no such decisive force as has been maintained; that a given perspective persists, on the average, for about three seconds, and that the average time of persistence differs slightly for the right and the left eye and for binocular regard. The rate of these perspective fluctuations would connect them more closely with those occurring in barely perceptible sensations, and would distinguish them from the periods found by experiments upon subjective rhythm.

As regards the cause of these changes of perspective, the author feels that the evidence is against a psychological explanation and in favor of a psychophysical. 'Psychophysical' appears to be interchangeable, in the author's mind, with 'physiological,' and in the end he lays considerable stress upon eye-movements and upon certain mysterious changes in the retina, that help to throw the perspective from one to another form. The theoretical discussion impresses one throughout as deficient in scientific nicety. On the whole, the book is an example of labor which is not without value, but which is in need of better intellectual guidance and skill.

G. M. Stratton.

Johns Hopkins University.

## AFTER-IMAGES.

*Bewegungsnachbild und Bewegungskontrast.* A. v. Szily. Zeitsch. f. Psych. u. Phys. d. Sinn., 1905, XXXVIII., 81–154.

The author repeated all of the experiments which are usually made in investigating the phenomena of after-images of motion, varying the conditions of these experiments and carrying them into considerable detail. His general conclusions are, first, that the appearance of motion in the after-image following the perception of motion is due to a purely physiological after-image of motion, and second, that the seat of this physiological process is somewhere central to the retina.

In his first experiments he moved series of parallel lines across the field of vision, either by actually moving the objective figure or by turning the eye in pursuit of a fixation point which traveled across the parallel lines. The after-image appeared to move in a direction opposite to that of the primary motion. When the lines were oblique to the direction of motion, the movement in the after-image was in a direction opposite to the apparent motion of the lines — that is, per-

pendicular to them. A series of angles whose sides were parallel and whose vertices were in a straight line seemed to grow more obtuse or more acute in the after-image according as the eye moved into the angles or the opposite. He noticed in these experiments that the after-image was confined to that part of the field corresponding to the part of the retina stimulated and that its position was determined by the position of the eyes. In an experiment to obtain distraction of the attention from the primary motion he presented a series of numbers which changed at short intervals at the center or fixation area of a spiral disk and which furnished a new problem in addition at each change. Under these conditions the primary motion of the spiral was not consciously perceived, but the after-image was. This proved that the after-image of motion could be obtained without the conscious perception of the primary motion.

This group of experiments led the author to the conclusion that the appearance of motion in the after-image is a physiological rather than a psychical process; first, because the after-image may be produced by any movement of the objective field across the retina — that is, either by eye movement or by movement of the objective figure; second, because the after-image is in constant relation to the part of the retina stimulated, and third, because it may result from the stimulation of the retina without conscious perception.

In a second series of experiments the maximum speed of movement in the object which would be followed by an after-image of movement was reached in the case of the rotating disk when the outlines began to flicker, that is, at the upper threshold for the perception of primary motion, and similarly the lower threshold was found to be nearly the same as that for the perception of the primary motion. The author found also that a true after-image of motion was produced by appearances which do not result from a genuine motion in the primary field. Such appearances were given in the stroboscope and by two series of parallel lines moving at right angles to each other, which give the appearance of a diagonal movement. The author does not connect these facts with his general conclusion that the nature of the after-image of motion is purely physiological, and the question arises as to whether they do not suggest a coöperation of the psychical factor in the production of the after-image.

In a third series of experiments the author gave attention to a veil-like appearance which at times appeared over the primary field, moving in the same direction as the appearance of motion in the after-image. With a darkened projection ground the after-image took on a similar

appearance, though when the ground was lighted sufficiently to make its contours or pattern visible the veil disappeared and the pattern took on the motion of the after-image. This convinced the author that the phenomenon was a genuine after-image of motion.

In a series of experiments to determine the seat of the after-image he found that the perception of motion through one eye with the other eye closed produced an after-image when the first eye was closed and the second open. He also found that when two opposite movements were presented, one to each eye, the after-image obtained with both eyes open was confused, but that a clear image was obtained from each eye separately, corresponding in general to its presentation, but modified by the character of the presentation to the other eye. These evidences that the after-image from one eye was influenced by the presentation to the other one was held as justification for the hypothesis that the seat of the after-image was central to the retina.

In general conclusion the author says in regard to the phenomenon: "This is no illusion of the judgment; indeed, one may not even call the appearances sense illusion, for they are presented necessarily in qualitatively and quantitatively predeterminate form; they are under the same conditions always the same and they appear even when the objective excitation which produces them does not come to conscious perception. My researches have shown that this objective excitation is given every time the immediate impression of a regular movement has lasted a considerable time."

The article is prefaced by a full historical review of previous experiments and of the theories advanced in explanation of the phenomena.

FRANK N. FREEMAN.

YALE UNIVERSITY.

## ATTENTION.

*Aufmerksamkeit und Zeitverschiebung in der Auffassung disparater Sinnesreize.* WILHELM PETERS. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, 1905, XXXIX, 401–428.

The object of Peters' experiments was to discover the effect of attention upon the time required to isolate the light-sound stimuli and incidentally harmonize the values found by Exner and Miss Hamlin.

With 'indifferent' attention to stimuli, values were obtained from three observers and used as the basis of comparison throughout. The sign + after the interval signifies that the light stimulus preceded by that amount in thousandth-seconds; the sign — that it followed. The limiting values were as follows:

| | | | |
|---|---|---|---|
| 'Earlier' | I. | 60 + to 35 + | 'Later' 5 — to 40 — |
| | II. | 45 + to 25 + | 35 — to 40 — |
| | III. | 80 + to 65 + | 10 — to 30 — |

Close attention was next given to the auditory stimulus with results as follows:

| | | | |
|---|---|---|---|
| 'Earlier' | I. | 95 + to 20 + | 'Later' 25 — to 125 — |
| | II. | 20 + | 25 — |
| | III. | 110 + to 105 + | 10 — to 65 — |

A comparison shows the 'earlier' limen to have been raised for I. and III. but lowered for II. The 'later' limen was raised for I. and III. but lowered for II. Close visual attention gave the following values:

| | | | |
|---|---|---|---|
| 'Earlier' | I. | 50 + to 30 + | 'Later' 25 to 90 — |
| | II. | 10 + to 5 + | 70 |
| | III. | 45 + to 10 + | 10 to 70 — |

Changing the experimental conditions by using convex lenses which made the visual stimulus indistinct, and at the same time required visual attention, the results were as follows:

| | | | |
|---|---|---|---|
| 'Earlier' | I. | 50 + to 35 + | 'Later' 25 — to 40 — |
| | II. | 15 + to 5 + | 45 — to 55 — |
| | III. | 80 + to 70 + | 20 — to 45 — |

Under these conditions the values obtained for indifferent and auditory attention scarcely differed from those obtained under previous normal conditions.

The remainder of the experiments were made upon a single observer — the III. of those preceding. The intensity of the sound stimulus was reduced until it could be distinguished only by close attention, then the visual stimulus was treated in the same way. The values under the first condition were:

'Earlier' 130 + to 95 + 'Later' 10 + to 55 +

Under the second,

'Earlier' 75 + 'Later' 40 —

The time displacement of involuntary concentration of the attention was next taken up. Such attention was secured by signal stimuli of the same kind as that to which attention should be given, but on the whole he concluded that these signal stimuli were distracting. The first values are for sound signals, the second for light:

| | | |
|---|---|---|
| 'Earlier' | 75 + to 30 + | 'Later' 10 — to 50 — |
| | 120 + to 80 + | 25 — to 65 — |

Instead of one pair of stimuli, as light-sound, a succession of light-sound stimuli were used. The values given are for (1) indifferent, (2) auditory, (3) optical, (4) optical with optical accommo-

dation, (5) indifferent with optical accommodation, (6) auditory signal stimuli, (7) optical signal stimuli.

| | | | |
|---|---|---|---|
| 'Earlier' | I. 100+ to 90+ | 'Later' | 30— to 5— |
| | II. 135+ to 125+ | | 100+ to 30+ |
| | III. 50+ to 40+ | | 85— to 80— |
| | IV. 105+ to 85+ | | 50— to 45— |
| | V. 75+ | | 35— (?) |
| | VI. 85+ to 75+ | | 105— to 5— |
| | VII. 120+ to 100+ | | 45— to 15+ |

Finally, these results are brought into harmony with the Wundt-Külpe, and the Exner facilitation and restraint theory of attention by saying that these results show a 'natural' attention — an unconscious, individual preference for visual or auditory stimuli.

GEORGE L. JACKSON.

UNIVERSITY OF MICHIGAN.

*Zur experimentellen Kritik der Theorie der Aufmerksamkeitsschwankungen.* BERTIL HAMMER. Zeitschr. f. Psychol., 1905, XXXVII, 363–376.

*Die Aufmerksamkeitsschwankungen.* C. E. SEASHORE. Ztschr. f. Psychol., 1905, XXXIX, 448–450.

Hammer holds that the so-called fluctuation of attention to visual intensity-differences is completely explained by retinal fatigue, and fixation changes which subject fresh retinal areas to stimulation. In an experiment he employed strips of gray paper laid in pairs on a gray screen, and found, in the one set of results given, that both phases of the period of fluctuation lengthened with increase in the difference between the two grays, and decreased with the continued repetition of the observation of a given difference. This result is supposed by Hammer to confirm his theory.

The theory involves the supposition that in auditory objects no fluctuations of attention occur. Hammer's second experiment showed accordingly that in the sound of a succession of strokes of an electric hammer, controlled by a metronome and supposed to strike uniformly, no fluctuation was observable. The alleged occurrence of auditory fluctuation reported by various experimenters is explained out of hand as due to faulty methods, presumably the use of unsteady sounds.

Seashore, in commenting on Hammer's article, says that the experiment on visual fluctuations brings out nothing which was not already well known. He criticises the report of the auditory experiment for omitting important information as to the rate of the metronome, the nearness of the sound to the threshold, and the duration of

the experiment. Assuming that the rate was near one per second, he would have predicted that no fluctuation would occur, since the attention wave would rise regularly to a crest with each stroke.

As an instance of faultless experimentation, in which fluctuation was clearly found, Seashore cites his own observation on fifty-five unsophisticated students, each of whom discovered the fluctuation in the sound of a chronometer beating fifth seconds, and in which results showed there was no considerable physical variation.

KNIGHT DUNLAP.

UNIVERSITY OF CALIFORNIA.

## FATIGUE.

*Recherches sur la fatigue intellectuelle scolaire et la mesure qui peut en être faite au moyen de l'esthésiomètre.* A. BINET. Année Psychologique, 1905, XI, 1–37.

This research concerning the effect of intellectual fatigue on sensibility was carried out by a commission appointed from the members of the 'Société libre pour l'étude de l'enfant.' The work consisted entirely of experiments on children in the primary schools of Paris.

The æsthesiometric method was used in all the experiments in which touch sensibility was tested. The back of the hand, screened from the view of the subject, was the part stimulated. In order to stimulate the skin by two points separated by a fixed distance, two needles were sunk into the edge of a card in such a way that the projecting ends of the needle could be lowered simultaneously upon the skin. There were seven such cards, the distances between the points being as follows: 0 cm., 0.5 cm., 1 cm., 1.5 cm., 2 cm., 2.5 cm., 3 cm.

The first tests were made on 45 boys and 38 girls from six to twelve years of age. The points were applied to the skin according to the method of maximum variation, that is, so that there would be the greatest possible contrast between the successive stimulations; 56 tests (*i. e.*, eight with each of the seven cards) were made before school in the morning; at the end of an hour of composition and arithmetic another similar series of tests was made.

As the average threshold for discrimination seemed to be about 1.5 cm. the conclusions concerning the effect of fatigue on touch discrimination were based on the relative number of right and wrong cases when the distances were 0.5 cm., 1.0 cm. or 1.5 cm. Out of the 840 tests on the boys recorded at these three distances there were 322 right judgments (two points). From the same number of tests

after fatigue there were only right 282 judgments, a decrease of 5 per cent. The decrease in the case of the girls was 11 per cent.

In the second experiment twenty boys and seventeen girls were tested in the morning before school work had begun and again after eight and one half or nine hours of intellectual work. A decrease of about 13 per cent. in the number of right cases was found for the boys and a decrease of about 14 per cent. for the girls.

A control was introduced in connection with this second experiment, to determine whether the loss of sensibility in the fatigue experiment could be due to loss of interest in the experiment. Tests were made on ten boys and ten girls on two successive mornings before their school work began. There was found to be practically no change in sensibility on the two occasions.

From these results Binet concludes that the decrease of sensibility is actually due to intellectual fatigue.

Binet's control experiment would seem open to criticism, since he has not simply reproduced the conditions of the other tests minus the fatigue factor. The first test of the second experiment was taken in the morning, the other eight and one half or nine hours later on the same day. The first test of the control was also taken in the morning, but the second was taken the next morning. Apart from fatigue there are several causes which might modify the discrimination sensibility, those causes, for instance, which appear in the work on fatigue by Kraepelin, Lindley and others (Antrieb, Anregung, physiological rhythms), etc.

The third experiment is the only one which was done under the direct supervision of Binet. This experiment, which was made on girls in the first class of the primary school, was very similar to the first except that only three stimuli were used, the distances between the pins being 0, 1, and 2 cm. The results showed again a decrease of sensibility.

Concerning the remainder of the paper, which takes up the question of the effect of fatigue on pain sensibility, little need be said, as the results are not conclusive.

The conclusions drawn from the entire work would seem to be that intellectual fatigue decreases touch sensibility and may perhaps have a similar effect on pain sensibility.

GRACE FERNALD.

BRYN MAWR COLLEGE.

## VISION.

*Ueber monoculares körperliches Sehen nebst Beschreibung eines als monoculares Stereoscop benutzten Stroboscopes.* M. STRAUB. Zeitsch. f. Psychol. u. Phys. d. Sinn., 1904, XXXVI., 431–439.

Stereoscopic effects, Straub maintains, are to be accounted for in the same way, whether they are binocular or monocular. A physiological explanation is out of the question in the latter case, however, because the double images and disparate points fall on a single retina, and consequently monocular fusion must be attributed to a mental act of inference. But if the mind can translate parallactic differences into depth difference in monocular vision there is no reason why it cannot do the same when the images fall on the two retinæ and there is no use in seeking a physiological explanation.

The first point is that monocular stereoscopic effects can actually be obtained in the use of the ophthalmoscope. With sufficient practice depth differences in the background of the eye which can ordinarily only be inferred by the observation of parallax stand out in relief. If, then, this effect arises from the unconscious mental combination of successive parallactic images, it ought to be seen when successive stereographs are viewed with one eye, and Straub finds that such is the case. He made sets of drawings for the stroboscope, such that two successive pictures made a pair of stereographs depicting a cube or other geometrical solid, and found that the monocular stereoscopic effect obtained was in no wise different from that obtained when the stereographs were observed in the usual way. By varying the conditions of this experiment and by further analyzing the appearance of relief which he observed with the ophthalmoscope, Straub was driven to the conclusion above stated, that in monocular vision parallax is actually translated by an unconscious mental process of fusion into a presentation in three dimensions. But when once the possibility of such a mental act has been admitted there is no way of escaping the further conclusion that stereoscopic effects in general may be explained on a purely psychological basis without taking any account of physiological theories. WARNER BROWN.

UNIVERSITY OF CALIFORNIA.

*Vergleichende Bestimmungen der Peripheriewerte des trichromatischen und des deuteranopischen Auges.* ROSWELL P. ANGIER. Zeitsch. f. Psychol. u. Physiol. d. Sinnesorgane, 1904, XXXVII., 401–413.

In view of the variation produced in ' periphery values ' by a more or less complete adaptation to light, the experiments of v. Kries on

Professor Nagel are repeated during adaptation produced by gazing at white clouds with an artificially dilated pupil. Under these circumstances the 'periphery values' for the 'dichromatic' eye differed systematically from that of the normal eye — the brightest part of the spectrum lying decidedly toward the red. A similar displacement, though less accurately measurable, was observed in foveal vision of the 'dichromatic' eye.

*Ueber Fixation in Dämmerungssehen.* RICHARD SIMON. Zeitsch. f. Psychol. u. Physiol. d. Sinnesorgane, 1904, XXXVI., 186–193.

The position of Mariotte's blind spot and the apparent position of negative after-images are used to determine the position of the eye in 'fixating' faint stimuli in the dark. While the experimental methods preclude any great degree of accuracy, and the generalizations are derived from the experience of only one subject, the measurements are the best at hand and constitute a decidedly suggestive contribution. Simon finds no considerable eye movements during the extra-foveal 'fixation.' There is nothing approaching nystagmus. Approximately the same part of the retina functions as center under similar circumstances; but this part varies for different eyes and varies for the same eye, though in the same meridian, with the degree of dark adaptation and the amount of illumination. RAYMOND DODGE.

WESLEYAN UNIVERSITY.

## PSYCHOPHYSICAL METHOD.

*Ein Beitrag über die sogenannten Vergleichungen übermerklicher Empfindungsunterschiede.* Jos. FRÖBES. Zeitsch. f. Psych. u. Phys. d. Sinnesorgane, 1904, XXXVI., 241–268; 344–380.

The specific point in this problem was to analyze the judgment actors in the perception of differences. The constant method, according to Müller, was followed to find a mean. Three weights, $A$, $B$ and $C$, were placed before the observers, who were to compare the difference between $B$ and $C$ with the difference between $A$ and $B$. There were two classes of results. In the first, being the results of a well trained observer, the differences were practically constant from day to day and approximately equal. In the second, the observers being not so well trained, the difference between $B$ and $C$ increased from day to day until the actual difference between $B$ and $C$ was about equal to the difference between $A$ and $B$. As to the judgment factors, the introspections and special tests on two observers showed two main factors, namely, comparison and absolute impression. In one ob-

server the latter predominated, and in the other both were present.

By similar methods a series of experiments was made using gray discs instead of weights. Among the darker discs the apparent mean approaches the geometrical mean more closely than the arithmetical mean, while among the brighter discs the apparent mean is greater than the arithmetical mean. The authors ascribes this to the stronger tendency of the brighter discs to attract the attention and thus shift the apparent mean toward them.

DANIEL STARCH.
UNIVERSITY OF IOWA.

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*La psychologie des individus et des sociétés chez Taine.* P. LACOMBE. Paris, Alcan, 1906. Pp. 375. 7 fr. 50.

*Les notions d'essence et d'existence dans la philosophie de Spinoza.* N. RIVAUD. Paris, Alcan, 1906. Pp. 216.

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*La classification des enfants anormaux.* O. DECROLY. Gand, Van der Haegen, 1905. Pp. 76.

*Organisation des écoles et institutions pour les arrières pédagogiques et médicaux.* O. DECROLY. Bruxelles, Bulens, 1905. Pp. 23.

*Les frontières anthropométriques des anormaux d'après M. Binet, appliqués à des enfants de Bruxelles.* O. DECROLY. 'Annales' of the Royal Society of Brussels, XIV., 2, 1905. Pp. 36.

*Ueber die Erfahrungsgrundlagen unseres Wissens.* A. MEINONG. (Abhandl. z. Didaktik u. Philos. d. Naturwiss., H. 6.) Berlin, Springer, 1906. M. 3.

*Proceedings of the Society for Physical Research.* Part LI., Vol. XIX. London, B. Johnson, 1905.

*Le problème du devenir et la notion de la matière dans la philosophie grecque depuis les origines jusqu'a Théophraste.* CH. RIVAUD. Paris, Alcan, 1906. Pp. viii + 488. Fr. 10.

*The Aims of Religious Education.* Proceedings of the Third Annual Convention of the Relig. Educ. Assoc. (Boston, Feb., 1905.) Chicago, The Association, 1905. Pp. 524.

*Sociology and Social Progress.* T. N. CARVER. Boston, Ginn & Co., no date. Pp. v + 310.

*Dictionary of Philosophy and Psychology.* Edited by J. MARK BALDWIN. Vol. III., Parts i and ii; bound separately. Bibliographies of Philosophy and cognate subjects, compiled by B. RAND. New York and London, Macmillans, 1905 (which should be 1906). Pp. xxiv + 542, and 543–1192. $10 net.

[The editor of these volumes wishes it known that he is not responsible for errors in the portions of which he did not see proofs, including the last 200 pages and the front matter, nor for the delays in their appearance of over a year that was not at all necessary, the type-setting having been 'held-up' for months by command of the publishers, with the 'copy' in the printing office, nor for the policy of withholding 'review' copies, nor for the advance in price of these and the other volumes to a figure prohibitive to many, a matter of which he was not even informed. His suggestions as to the division and sub-titles of the two parts of the volume were also ignored. One of the errors of sufficient importance to note is on the title page: Dr. Rand, the compiler of the bibliographies, has not been connected with Harvard University for some time. Furthermore, the publishers, besides printing a 'paying' edition, 2000 copies,[1] of the entire work, have had *papier maché* plates made for reprinting and refuse to discuss the subject of a revised edition. The editor has spent too much labor on the work to consent to delegate entirely to the publishers its future supervision and fortunes, and further plans will soon be announced.—J. M. B.]

[1] Of which but 375 of Vol. I. and 544 of Vol. II. were left in May, 1905, according to their report.

# NOTES AND NEWS.

IT is announced that with the beginning of the new volume (41) the *Zeitschrift f. Psychologie u. Physiologie d. Sinnesorgane* will be divided into separate issues, called respectively *Zeitschrift f. Psychologie* (to be edited by Ebbinghaus) and *Zeitschrift f. Sinnesphysiologie* (to be edited by Nagel). The Literature Section will not be divided, but will appear as a department of the *Zeitschrift für Psychologie* only, although covering the literature of 'Sinnesphysiologie' as well. Each of the two journals will be the customary size, and each will be sold separately (15 M. the volume). With the current number of the *Zeitschrift* appears the *Bibliographie* for the year 1904, just one year later than our own INDEX, in coöperation with which it is compiled. We congratulate the editors of the *Zeitschrift* upon the completion of forty valuable volumes. — ED.

PROFESSOR ROBERT FLINT has been appointed Gifford Lecturer for a two years' term at Edinburgh and Professor James Ward for a three years' term at St. Andrews. Philosophers are quite ready for further Gifford Lectures from Professor Ward.

A CALL has been issued for a 'Kongress für Kinderforschung und Jugendfürsorge,' to meet in Berlin early in October. All interested in the study, protection, and education of childhood and youth, whether physiologists, psychologists, biologists, or teachers, are invited to take part. While the congress is intended especially for German-speaking countries, the attendance and participation of foreigners is welcomed. A fee of 4 Marks is fixed for participants, in return for which the Proceedings will be sent when printed. A large committee of arrangements is named, the principal officers being Dr. W. Münch (Berlin) president, J. Trüper (Jena) vice-president, and Dr. W. Ament secretary; the address of the secretary is Sanderglacisstrasse 44, Würzburg.

A PRELIMINARY circular has also been issued looking to the formation of an International Congress for Child Study to supplement the German and other national congresses.

PROFESSOR G. A. TAWNEY, of Beloit College, Wisconsin, is taking charge of Professor G. S. Fullerton's work at Columbia University during the latter's leave of absence covering the present semester.